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THE AMERICAN

METEOROLOGICAL JOURNAL.

VOL. VII. ANN ARBOR, JUNE, 1890. No. 2.

ORIGINAL ARTICLES.

LAWS OF THE DISTRIBUTION OF CLOUDINESS OVER THE SURFACE OF THE GLOBE.

BY M. LÉON TEISSERENC DE BORT.*

Until within a few years the distribution of cloud over the globe had not been made the subject of any general study, and there were only some researches upon special regions such as those by MM. Wild, Woeikoff, and Elfert. M. Renou published, in 1879, the first chart representing by isonephs, or lines of equal cloud amounts, the annual distribution of cloud over Europe, with some brief data for the whole globe. The writer has undertaken to fill the gap which existed in our meteorological knowledge by preparing charts with the isonephs for each month of the year and for the year itself. This work, which required much calculation, is based upon the observations at more than 700 continental stations, to which were added the discussion of 108,000 observations made on the Atlantic Ocean. It has thus been possible to establish for the first time the laws of the distribution of cloudiness over the surface of the globe, which are as follows:

1. In all months there is a well-marked tendency for the cloudiness to arrange itself in zones parallel to the equator.

2. When the distribution of cloudiness is separated from the perturbations which complicate it, in order to regard only the general phenomena, there are seen to exist: (a) a maximum of

* Translated from M. de Bort's manuscript by A. Lawrence Rotch.

cloud at the equator, changing its position slightly according to the sun's declination; (b) a band of little cloud between 15° to 35° of north and south latitude; (c) a zone of clouded sky from 35° to 50°, while higher, judging by what occurs in the northern hemisphere, the sky becomes clearer towards the poles.

The conditions which cause perturbations in this general distribution are the following:

1. *Other things being equal, the cloudiness is much less over the continents than over the oceans.*

2. *Every elevated coast exposed to a prevailing sea wind gives rise to a maximum of relative cloudiness.*

3. *Every region occupied by the sea, where a continental wind prevails, has a relative minimum of cloudiness.*

4. *A wind which passes from a warm region to a colder one causes an increase of cloudiness.*

We give below the mean values of cloud over the whole earth for each five degrees of latitude for four characteristic months of the year:

MEAN CLOUDINESS OVER THE GLOBE.

Latitude.	Northern Hemisphere.				Southern Hemisphere.			
°	Jan.	Apr.	July.	Oct.	Jan.	Apr.	July.	Oct.
70	5.5	5.6	5.9	6.8	...	...	...	...
65	5.6	5.4	6.1	6.7	...	...	...	...
60	6.2	5.7	6.2	6.8	7.0	7.0	7.0	7.0
55	5.9	5.9	6.1	6.7	7.0	6.9	6.7	7.0
50	5.7	5.7	5.7	6.1	7.0	6.5	6.2	7.0
45	5.2	5.2	5.0	5.3	6.0	5.8	5.4	6.0
40	5.0	4.9	4.4	4.8	5.3	5.5	5.6	5.6
35	4.6	4.6	4.1	4.3	4.8	5.0	5.2	5.1
30	4.4	4.2	4.2	4.0	4.7	4.7	4.7	4.8
25	3.7	4.0	4.5	3.7	4.8	4.7	4.6	4.7
20	3 7	3.8	5.0	4.1	5.1	4.8	4.3	4.8
15	4.0	3.9	5.3	4.8	5.3	5.1	4.8	5.1
10	4.5	4.5	5.9	5.3	5.8	5.8	5.1	5.5
5	5.0	5.2	5.9	5.6	5.6	5.5	5.7	5.8
0	5.0	5.6	5.8	5.6	...	...	...	...

GENERAL MEANS.

	Jan.	Apr.	July.	Oct.
Northern Hemisphere, 0° to 70°, . .	4.8	4.8	5.3	5.2
Southern Hemisphere, 0° to 60°, . .	5.5	5.5	5.4	5.6
Globe, 60° N. to 60° S., ...	5.1	5.1	5.3	5.3

We will now examine in detail the causes which rule the distribution of cloudiness over the globe.

Causes which Influence the Distribution of Cloudiness.—A study of the charts of isonephs compared with those of the

other meteorological elements shows that the cloudiness depends chiefly upon the vertical component of the air. It is, indeed, evident that a mass of air which rises in consequence of the arrangement of surfaces of equal pressure in the atmosphere, by the cooling due to expansion, reaches a temperature sufficient to condense the vapor contained in it. It follows that above regions where there are frequently low pressures, the cloudiness should be great; on the contrary, above regions of high pressures which are the seat of descending currents, a clear sky should be found. This is in fact what is observed. (See Note).*

It likewise follows that the presence of any obstacle to the horizontal motion of the wind, such as an elevated coast or a mountain, favors the production of clouds. Also, when the relative height of the obstacle is sufficient to cause condensation of the aqueous vapor we see a cloud maximum produced on the slope facing the wind, whilst the maximum occurs behind the obstacle, the clouds being stopped by the mountain and the wind in following the ground having a descending movement imparted which warms it and again dissipates the condensed vapor. A striking example of this phenomenon can be seen on the Norwegian coast, especially in the months of January and February, where the greatest cloudiness is found on the west coast which is very high and protected from the ocean winds, whilst the slopes which constitute Sweden have a much clearer sky. Among the causes of cooling of the air there should be included the changes of latitude which bring the wind in contact with regions warmer or colder than its place of origin. In the first case the cloudiness increases, in the second it diminishes. This is often noticed in contrasting the effects of north and south winds.

Effects of the Continents.—The continents which yield much

NOTE.—The cloudiness gives us a valuable indication of the decrease of temperature which has such an important influence on the circulation of the atmosphere. As soon as there is condensation in the ascending current, the decrease of temperature with the height diminishes in a large proportion. If we recall the principle demonstrated by M. Peslin, and now admitted in meteorology, that the air in its ascensional movement gives rise to an expenditure of energy there where the hygrometric conditions are such that it cools less rapidly in rising than the surrounding air, we are led to the conclusion that *other things being equal, the regions where the relative humidity is such that the sky is very cloudy, are those where the ascending movements of the air have the greatest chance of continuing.* The ascending movement thus produces clouds, and the heat set free by the condensation in checking the decrease of temperature tends to maintain the ascending movement. In very dry portions of the earth the condensation of the aqueous vapor only takes place at great altitudes and the ascending movements of the air do not tend to increase.

less water vapor than do the oceans, exercise a considerable influence upon the distribution of cloud and it is their action which gives to the theoretical arrangement of the isonephs the greatest perturbations as they also do to the isobars and isotherms. For example, we can see in the northern hemisphere, where the continents have a considerable extent, that the zone of much cloud is broken above North America and Asia. There are two reasons for this: (1) The surface is less moist than the sea; (2) the influence of the continents on the temperature and on the isobars often causes descending air currents. For example, in Asia, near Yakoutsk, which in winter should be in the zone of much cloud, the sky is generally tolerably clear. A simple inspection of the chart of isotherms and isobars shows that in winter there exists in these regions an area of high pressure and a descending movement of air which prevents much cloud formation. The continents during the warm season generally produce by their heating an indraught of air from the adjoining countries, and quite intense ascending movements are thus established. It would appear, consequently, after what we have already said, that the sky ought to be cloudy, but this is only true in the equatorial zone and at certain points in high latitudes, briefly, where the temperature of the land does not sufficiently exceed the temperature of the ocean or of the neighboring regions, to heat the air and thus counteract the cooling effect. Algeria and all the northern coast of Africa have a tolerably clear sky in summer, and the same is true of the largest portion of Australia, Africa and South America, because there the air being heated greatly on the ground the ascending movement which constitutes a real convergent monsoon is not sufficient to bring about condensation. Moreover, it should be noted that these countries are situated near 35° latitude, where the air which escapes from the areas of high pressure is in a less humid condition than in the other regions, thus rendering condensation more difficult. Near the equator itself the ocean is very warm and the air is nearly saturated so that every ascending movement caused by indraught or by an obstacle to the general motion of the air should be accompanied by a great increase in the cloudiness.

Annual Variation of Cloudiness.—The examination of charts of isonephs, from the point of view of the annual variation of cloud, shows that this element has a nearly constant value over the atmospheric centers of action, as is seen

especially by the cloud maxima north of the oceans and by the minima in Asia, in Central America, in South Africa and in the Argentine Republic. The equatorial maximum varies more in its position, because it is a direct result of the sun's declination. The annual variation of cloud above the great centers of action is much less marked than that of the temperature and pressure, because on the continents of Asia, America, etc., the change of direction in the isonormals of temperature from winter to summer determines the formation of maximum pressures in the cold season and of minima in the warm season, and, consequently, of a barometric fluctuation which may amount to, perhaps, an inch, whilst the cloudiness in either case is always small on account of the hygrometric state of the air which does not permit abundant condensation of the water vapor. But the annual variation of the quantity of cloud is the greatest possible around these centers of action and especially around those which we have designated *reversible* maxima and minima, because monsoons are there produced which are sometimes land and sometimes marine winds giving rise to diametrically opposite effects. In this respect, India is the country where the change is most complete. In winter the sky is very clear there and the cloudiness is from 20 to 30 per cent. during the northeast monsoon, which is nothing else but a regular trade-wind, reinforced by the cold of the high plateaux of Asia. In the month of June, on the contrary, the heating of the Asiatic continent having caused a flow of air from all parts, the southwest monsoon is established, and the arrival of the damp sea air on the mountain ranges and high lands of India, which forces it to rise, causes the formation of numerous clouds. It is the same in Mexico, where in winter a clear sky predominates and in summer a cloudy one, at least in the countries which are directly subject to the action of the sea winds.

In the northern part of the oceans and especially on the Atlantic and Pacific, which we know sufficiently well, the cloudiness remains great all the year, although the barometric minimum which obtains in the cold season is almost completely obliterated in summer. From the above one might be tempted to conclude that as the ascending movement of the air diminished the sky should become much clearer. But the inspection of the charts of isobars, winds and isotherms demonstrates: (1) that the prevailing southwest winds are also ascending winds; (2) that frequent cyclones cross these regions, and

although they are less severe than in winter, they bring cloudy weather.

Over the whole globe November is the month when the sky is the most covered. In fact, if we remark that, in consequence of the stability of the position of the centers of action of the atmosphere in the southern hemisphere, this portion of the earth has an almost constant cloudiness on the whole, so the greatest variation in the quantity of cloud can only come from the northern hemisphere; and it is at the close of the autumn that the maximum cloudiness occurs in this hemisphere which results from several causes: (1) The great extent of the barometric minima over the oceans, the frequency and importance of the depressions, and the relative smallness of the continental barometric maxima which coincide with the descending movements and maintain the sky clear. (2) The existence at this season of a great amount of aqueous vapor which has accumulated in the air by reason of the active evaporation during the summer. (3) The high temperature of the sea which favors the production of aqueous vapor and plays a very important part in the formation of low pressures. (4) Finally, the fact must be remembered, that with the approach of winter the mean temperature of the atmosphere decreases, which again favors the condensation of the vapor.

The month of September is that in which the isonephs are most nearly parallel to the equator, which agrees entirely with the fact that this is the season when the distribution of temperature and pressure approaches most nearly the theoretical arrangement. The isonephs of this month have almost the same arrangement in parallel bands which we described in our general considerations, and which correspond to the theoretical distribution of cloud over a globe having a homogeneous surface.

The clearest month over the globe is March. (See Fig. 1).

At this period the high pressures keep the weather clear over the continents of the northern hemisphere, while the barometric minima are less persistent than in winter and often alternate with the areas of high pressure. Moreover at the close of winter the quantity of aqueous vapor contained in the air is small and the temperature is rising almost everywhere in the northern hemisphere, so that the relative humidity of the air and consequently the condensation of aqueous vapor are diminished.

The minimum cloudiness in the center of Asia and in Siberia

undergoes a well-defined annual variation both in extent and amount, although it always remains well-marked. The sky is clearest in February when the isoneph 2 encloses part of China

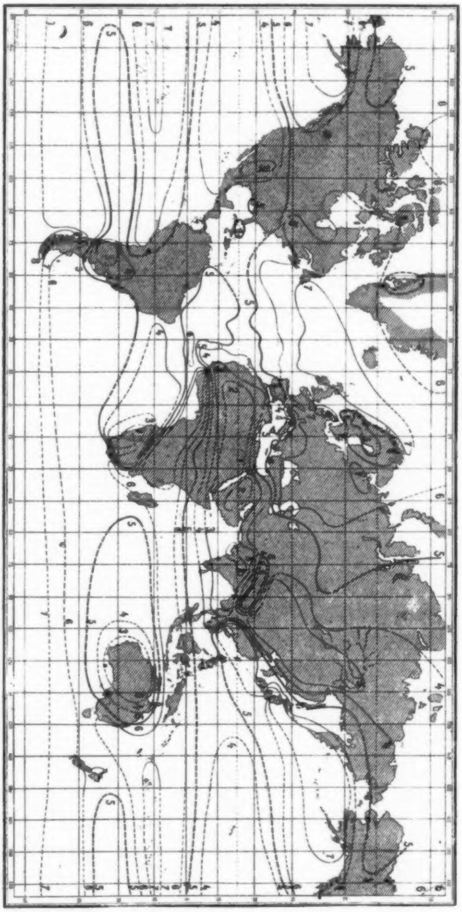


FIGURE 1. MEAN ISONEPHS FOR MARCH.

and the center of the mountainous region of Siberia, while the isoneph 5 follows the coast of the Pacific to the east and ascends towards Nova Zembla on the west. In India there is also a very clear sky separated only from the Siberian minimum by a

maximum of cloud over the Himalayas which is probably due to the condensation produced by contact with the glaciers. In June, when the ascending monsoon commences, the cloudiness increases considerably in Siberia and the isoneph of 5 descends towards latitude 50° N. During this time the sky becomes clearer over Turkestan, the Black Sea and the Caspian Sea regions, where the summer *régime* commences, as the moist air from the ocean cannot penetrate there. The cloudiness over the north of Africa also reaches its minimum in June, at a time when the difference of temperature between the Mediterranean, the Atlantic and the continent is greatest, so that the air whose humidity would have been increased in rising on the coast is at the same time heated by contact. In this region the maximum cloudiness occurs in December where the contrasts of temperature between sea and land are the reverse of what we have indicated for June. Figure 2 shows the mean isonephs over the globe for July, which on the whole is a cloudy month.

The equatorial maximum, as well as can be determined by the small number of observations in those latitudes, reaches its maximum in March, when it presents a narrow and tolerably uniform band. The region situated to the south of Tobolsk merits notice as being during the whole year the seat of a cloud maximum which is well shown by the observations at Barnoul, Tomsk, etc. M. Woeikoff has called attention to this maximum of the middle Oural and Altai mountains without explaining the cause. The following considerations will perhaps facilitate the explanation. In the first place, this region is precisely that which we cited in our study of the types of isobars as being the point by which the high pressures are often encroached upon by the depressions. The trace of the giving way of pressure is shown on the charts of mean isobars of the cold season where a well-marked bending is seen in this region. The presence of barometric depressions necessarily causes the existence of southwest ascending winds south of the center of these cyclones. Now the middle Oural and the plain north of the Altai are situated exactly where these southwest winds blow when a depression reaches these regions. In the second place, an examination of a map of the country shows the existence of a great number of small lakes which gives reason to believe that the rain water finds few issues, and, consequently, that the ground and the air remain moist at all seasons.

Analogy with the Other Planets.—As we have seen, the dis-

tribution of the cloudiness is, as a whole, a direct consequence of the variation of the wind and especially of the direction of its vertical component, and also of the humidity, although

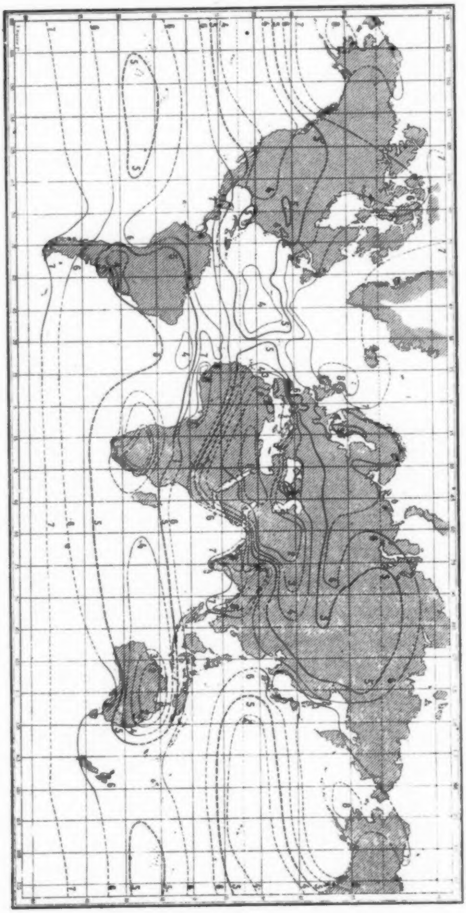


FIGURE 2. MEAN ISONEPHS FOR JULY.

this last only plays a ruling part in the extremes. The winds themselves depend upon the distribution of pressure which is thus the preponderating influence.

These same phenomena probably occur in their essential

features on the planets which possess an atmosphere, and the bands of clear or cloudy sky which exist on the earth ought to correspond to bands of the same kind which are observed on different planets. Seen from a point in space the cloud bands of the earth would appear brilliant, while the regions where the slightly clouded sky permits the soil to be seen would be darker, this aspect being well known to persons who have ascended on mountains or in balloons. If, accordingly, we picture our globe as seen in space, by representing the cloudiness by tints darker in proportion as the earth's surface is more visible, we obtain

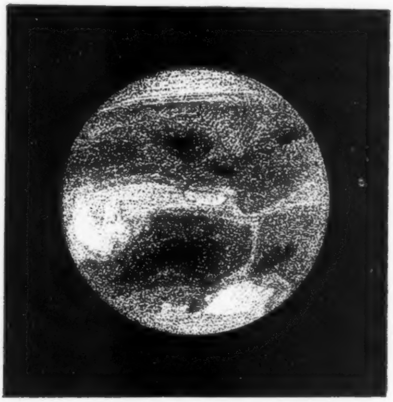


FIGURE 3. HEMISPHERE OF EUROPE, ASIA AND AFRICA.—JULY.

something like Figures 3 and 4. In order to render the analogy with the other planets more striking, we give in Figure 5 the aspect of Jupiter with his bands on April 21, 1886, from the photographs of MM. Henry of the Paris Observatory.

One can see in these photographs, as in most of the later ones taken with the greatest care by M. Trouvelot of the Meudon Observatory, and by M. Boinot of the Paris Observatory, that there exist upon Jupiter two darker bands near 15° and three light bands, one at the equator, the others near the tropics. On Saturn, direct observations show, as do the photographs, similar bands. Upon Mars no bands have been found, but from time to time the planet appears surrounded by clouds which prevent its surface from being distinctly seen. But then the researches of astronomers have been rather upon the configura-

tion of the surface of this planet, and not on the arrangement and amount of the clouds which appear there; thus it has only

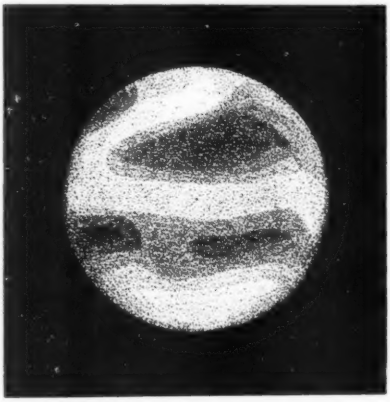


FIGURE 4. PACIFIC HEMISPHERE.—JULY.

been noted when the planet was most visible, so that it is unknown what circumstances affect the clouds on Mars. There are, moreover, probabilities based upon scientific reasons, that

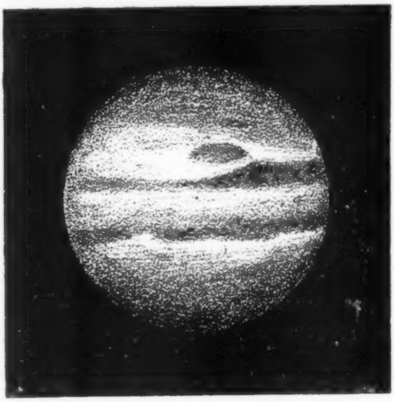


FIGURE 5. JUPITER.

the clouds upon Mars are not distributed in the same manner as upon the earth.

The distribution of the cloudiness (taking this term in its most general sense and admitting that clouds result from the condensation of the vapors of the liquids which are found on the planets, without defining the nature of these liquids) depends in a very large degree upon the distribution of the pressures, as we have previously demonstrated, and therefore it follows that the observation of the bands on the planets, if these bands are really due to clouds, as appears very reasonable, furnishes a means of studying from a distance the distribution of the pressure in the planets' atmosphere. The writer would direct * to these new ideas the attention of astronomers, because of the interest which would result from noting what occurs upon planets like Mars when the atmosphere is not sufficiently transparent to permit the planet's soil to be seen. The writer will again refer to the important consequences to meteorology which might result from these investigations.

IS THE DIURNAL VARIATION OF THE MAGNETIC NEEDLE A METEOROLOGICAL PHENOMENON?

By Professor Richard Owen.

If it can be shown that the paramagnetic oxygen of our atmosphere is the most probable proximate cause of the diurnal variation, the above query must be answered affirmatively, as affording at least a reasonable working hypothesis.

The object of this paper is to show that our atmosphere is the medium influenced magnetically by the sun, in affecting this diurnal movement.

Evidently the movement is connected with the sun, because the forenoon *westward* march of the north-seeking or positive pole of the needle, in our northern hemisphere, and the *eastward* march of the positive pole, in the southern hemisphere, commence soon after sunrise, at any given locality, and continue until the rotation of the earth has brought the sun's rays west of the magnetic meridian (whether the secular declination at that locality be east or west), when suddenly the movement is reversed in both hemispheres.

A mere difference in temperature will not explain the phe-

* The writer would be very grateful to persons who will be good enough to send photographs of the planets, or drawings made at different phases, to him at 82 Avenue Marceau, Paris.

nomenon, nor will the sun's attractive power, as claimed by some, accord with the facts, inasmuch as the effect produced, if derived from the sun, must be one of repulsion. When the sun rises, in middle latitudes, it is often 90° to the east of the magnetic meridian, and therefore, if that luminary attracted the needle, it ought to move to the east; whereas it at once commences going west, and continues this movement *in advance* of the sun, until that orb appears to pass the magnetic meridian, when suddenly the needle is repelled to the east, away from the sun.

We may next inquire (under this supposition that the sun is the remote cause of the needle's diurnal oscillation) what medium is used as the proximate cause, to effect this object; premising a few words regarding the earth's magnetism.

That the earth is rendered magnetic by induction from the sun is strongly indicated, if not proved, by relatively greatest magnetic intensity prevailing over our globe during perihelion; also by a periodicity in the three elements of terrestrial magnetism, in connection with maxima and minima sun-spot periods; besides the constant synchronism of storms in the photosphere, with auroral displays, and with the simultaneous disturbance of the needle at all the magnetic observatories.

Should the above view of terrestrial magnetism be accepted, then, according to the known laws of magnetism, the sun's northern hemisphere must necessarily be in the opposite magnetic condition from that of the earth's northern hemisphere; so also the southern hemispheres of both, with a neutral zone between, in or near their equatorial regions, modified somewhat by the sun's equator forming an angle of 7° with the ecliptic, and by the latter not altogether coinciding with the earth's magnetic equator.

To avoid the confusion of giving, either to the solar or the terrestrial northern hemisphere, the appellation of northern magnetism, when the two are in opposite conditions, we may call the magnetism of the sun's northern hemisphere *positive* and that of the earth's northern hemisphere *negative*, or plus and minus. The north seeking end of the magnetic needle, having often a short cross-bar, can therefore appropriately be indicated by a plus sign, while the unmarked end will be minus, as in positive and negative electricity.

If the sun is thus capable of rendering our globe magnetic, by induction, that luminary is certainly equally capable of mag-

netizing the oxygen molecules of our atmosphere. The oxygen, on the above view, (being only mechanically mingled with the nitrogen), would set paramagnetically in the earth's northern hemisphere, in lines coincident with the rays from the sun's northern hemisphere, having the minus pole of the oxygen molecule turned toward the sun's positive hemisphere, and the plus pole to the earth's negative hemisphere; consequently the oxygen presents the plus poles of its molecules to the east side of the needle; thereby repelling the needle's plus pole, and attracting its minus pole; while the molecules on the west side of the needle present to it their minus poles, which, by attracting the positive pole of the needle and repelling its negative pole, aid the molecules on the east in moving the needle to the west during the forenoon. In the afternoon the plus pole of the oxygen molecule being away from the sun is now on the west side of the needle, and repels its positive pole eastward, aided by the other molecules whose signs are all the reverse of those decribed as acting in the forenoon.

Oxygen has been shown by Faraday and Becquerel to be the most highly magnetic of all gases. The latter physicist estimates the power of a condensed cubic yard of oxygen as being equal to 5.5 grains of iron. [See Ganot's Physics, p. 751.] This would seem sufficient to oscillate the needle in middle latitudes $5'$ to $6'$ each side the mean; and, in higher latitudes (where the oxygen being colder is more magnetic) to cause an oscillation sometimes of 3° to 4°: inasmuch as the point of a cambric needle less than one-twentieth of an inch long, inserted into a cork, sufficed to attract a not especially sensitive needle from a state of repose when presented at a distance of one-tenth of an inch. This fragment of steel was estimated, by weighing a whole needle, to be equal to 0.022 of a grain, which would require sixteen cubic inches of oxygen as an equivalent. Four cubic inches of atmospheric air could readily inpinge on each side of each pole in a needle six inches long. The oxygen would only constitute one-fifth; but being in actual contact with the needle, instead of one-tenth of an inch distant, might have the necessary power to cause minute oscillations.

The phenomena in the southern hemisphere would be similar with reversed signs: namely, in our southern hemisphere, the sun's southern hemisphere has, by induction, rendered the oxygen molecules also paramagnetic, but with the plus pole to the sun, the minus pole to the east side of any magnetic needles in

our southern hemisphere, which point with their plus end northerly: thus the negative end of the needle is repelled, while the positive end is attracted; making the march of the positive pole of the needle an *eastward* movement in the *forenoon* and a *westward* march in the *afternoon;* exactly the reverse direction of the daily movements in the northern hemisphere.

We can readily see, also, why near the magnetic equator, (as at St. Helena, Singapore, etc.) the phenomena of diurnal variation exhibit movements corresponding with those in the northern hemisphere, while the sun is in the northern signs; but change to those of the southern hemisphere where the sun enters the southern signs.

It is equally easy to understand why the intervals between the turning points (which in middle latitudes are usually from about 8:15 A. M. to 2:45 P. M.) should in high latitudes, at certain seasons, when the sun's rays remain but a few hours above the horizon, be much shortened.

A simple illustrative experiment may aid us in understanding how the power of the oxygen molecules may be increased by solar influence.

If to the positive, or carbon pole of a bichromate cell, we attach a solenoid, or even a many stranded coil of No. 22 insulated copper wire, and bring the terminal end back to the negative, or zinc pole, and, facing the north, place the whole apparatus in front of us, then the space enclosed by the coil assumes the properties of a magnetic needle, as soon as a current of electricity environs that space by passing along the coiled wire.

When the current is made to pass thus *sinistrorsally* (from E over to W) the N aperture of the coil will so reverse a magnetic needle that its positive pole turns through 180°, and points due S into the enclosed field of strain, showing the N end of that field to be of negative magnetism. With the reversal of the wires, so as to produce a *dextrorsal* or right-handed current, the S aperture of the field will cause the positive pole of a magnetic needle to point due N through the aperture; hence this end of the field is now negative.

This latter effect must be the one produced by the sun on oxygen molecules in the northern hemisphere, if magnetism is, as Ampère supposed (and as this experiment seems to prove), a form of motion, among a few substances, such as iron, cobalt, nickel and oxygen, always perpendicular to the plane of the

so-called current of another mode of motion termed electricity.

If the sun produces electrical motion (which few physicists doubt) the latter, in the northern hemisphere, must environ the the oxygen molecules of our atmosphere *dextrorsally*, probably through the medium of the cosmic ether: thereby inducing plus magnetism in the pole of the molecule presented to our globe, while the minus end faces the sun.

In the southern hemisphere the current is propagated *sinistrorsally* around the oxygen molecule, so that its minus pole turns to our earth.

The above may aid us also in understanding why the storms in our northern hemisphere rotate from right to left, and in the southern hemisphere from left to right, while the storm itself always advances from lower to higher latitudes, probably attracted by the more strongly magnetic cold polar atmosphere.

It may be further useful to recall the fact that, in the above and similar experiments, the magnetic power of the enclosed space or field of strain (which seems due to the oxygen molecules therein being environed by electricity) is increased and diminished exactly in proportion to the strength of the electrical current.

NEW HARMONY, IND., February 25, 1890.

A STUDY OF CLIMATOLOGICAL INFLUENCE UPON THE RECENT EPIDEMIC OF INFLUENZA.

BY R. ASSMANN, M. D., BERLIN.*

(CONCLUDED.)

2. More important, still, than the confirmation of the fact of the almost total lack of precipitation of moisture, the certainty of this fact appears, that no lasting snow had fallen in Europe and Russia, and the quantities of snow were everywhere soon melted, either by rain or warmer weather; the immediate contact of the open dry soil with the winds blowing over it allowed the carrying off of great quantities of dust.

It is true, doubtless, that had there been even less rain a thin snow covering would have prevented the polluting of the air by dust.

3. In consequence of the slight precipitations of moisture the cleansing influence appeared only in an insufficient degree, so that the dust already in the atmosphere must have been increased.

4. In the greater part of the two monthly periods thus investigated, there prevailed a cloudy sky over almost the whole of Europe, or else a low lying more or less dense fog, which prevented a lessening of dust in the lower air strata, which otherwise would have spread out in larger space in higher regions.

5. The fog was caused especially by the predominance of a high, often a very high, pressure of air, and the consequent insufficient vertical exchange of air. For weeks we find, on the weather charts in November and December, districts where the barometer indicates more than 780 mm, often above 785, and at times as high as 788 mm. At the same time (that is in the beginning of December) it must not be overlooked that at the time of the invasion of the epidemic in central Europe, there prevailed in this district northeasterly, easterly and southeasterly winds, which promoted the spreading of the germ of the disease from Russia. Even though we find in the present short sketch important moments of the meteorological conditions of the months in which the influenza epidemic prevailed, and which moments may be called "promoting ones," there is yet, as we willingly confess, only a very small portion of obscurity banished which surrounds the disease. In the first instance, the authentication of the origin of the sickness and the study of the condition necessary for its existence would be of importance. Although there is strong probability that this is a specific bacillus, which under favorable conditions reaches a point of extraordinary development at the circumscribed place, and, taking its course with the prevailing currents of air, it propagates rapidly wherever it finds the same favorable condition for its growth and increase, still it would be rather daring to draw far reaching conclusions from these conjectures. Certainly it is the peculiarity of most of the so far known micro-organisms to grow best in a high temperature and moisture.

But should we draw the conclusion from this, that a propagation of the same in a low temperature and dryness would be improbable, we would overlook the fact that a mingling of the ideas of propagation and preservation might lead to a concealment of the state of the fact. Thus, for instance, the bacillus in question might thrive well in the damp and warmer summer months, but through frequent rains might have been carried to the ground and thereby prevented from entering the atmosphere, at least for a long time.

And the bacillus can endure very low temperature. We

know of several forms that live in water heated to the boiling point. There may begin with the dry period the migration of the micro-organisms which, although not further developed at the temperature, are still capable of being developed if they reach such places as grant them a second favorable ground for nutrition. That cities and all human dwelling places may be said to contain these nutritious soils seems credible. In large, densely populated cities, the epidemic is very prevalent, while in the country it only appears occasionally. That the cause which called forth this epidemic is not essentially dependent upon the temperature of the air is shown by the following, that in Russia the temperature which was repeatedly 20° and this proved as slight a check to the disease as a temperature of 10°. It is not surprising that after the harmful factor had once found its way into the homes, rainfalls or strong ventilation of the lower strata of air, as they appear in attendance upon barometrical depressions, do not bring the epidemic to an end.

Every place afflicted with the epidemic becomes a hearth or biding place for the disease, from which, under favorable conditions, quantities of bacilli spread. It would therefore be amiss to judge the state of the epidemic from the prevailing condition of the weather at any place. In spite of rain and the relatively low air pressure the epidemic raged at Brussels on the 22d of December, and during the time of severest frosts and highest barometer indications on the 28th of December in Austria. But as far as we can gain a true idea of the state of the epidemic from the daily papers we always find a deterioration of the epidemic, especially with reference to complications of pneumonia, where there is higher air pressure.

The close connection between increasing pollution of the lower atmospheric strata, through dust accumulation, with the rapid increase of lung affections and inflammations, especially pneumonia, the author has already tried to prove. In the fall of the year 1883 a very evident accumulation of dust in the atmosphere and an abnormally high barometrical maximum coincide with a corresponding sudden increase of the sick rates at Magdeburg.

In order to prove the foregoing views it would naturally be necessary to examine into the climatic conditions during former influenza epidemics.

But unfortunately the very desirable connection between medicine and meteorology which promises success is yet so

immature that an attempt of this kind would probably prove a failure because of the lack of reliable material. Still the author will endeavor to return to this question in a later and more explicit examination. The author, who is an active practicing physician of many years, and who for twenty years has ardently sought the relation between disease and climatic conditions, thinks it his duty to make use of the occasion [as the influenza disease possesses all the qualities of a real epidemic] to show briefly all those factors which allow a possibility of a combination with meteorological investigations.

From his own abundant experiences he ventures to express the wish for others not to draw far reaching conclusions from mere accidental statistic coincidences of heterogeneous phenomena without regard to the fact that these conclusions are in direct opposition to the fundamental doctrines of physics and physiology.

The conclusion *post hoc ergo propter hoc* has been a curse to medicine, to remove which our enlightening age still has to labor. Beware lest this curse attach itself to the heels of meteorology also.

NEW ENGLAND METEOROLOGICAL SOCIETY.

The eighteenth regular meeting of the New England Meteorological Society was held at Providence, R. I., April 15. The chief subject of discussion was "Climatic Changes," which were considered in two divisions: (1) Secular Changes, and (2) Supposed Recent Changes in Climate.

1. *Secular Changes in Climate.*—This was introduced by Professor W. M. Davis, of Harvard College, as follows: Climate deals with the average local values of recurrent atmospheric conditions, whose individual parts constitute the weather at a certain station. In some tropical countries the conditions of successive days are so much alike, and the days vary so little or so systematically through the year, that weather approaches close to climate. But in our latitude successive days are very unlike, and the seasons are strongly contrasted; here weather and climate are distinct enough.

The value of our climatic factors is determined by averaging the weather records for ten or twenty or more years. We thus obtain values that will correspond closely with the averages of the same kind of records for similar periods of time, but of ear-

lier or later date. The slight differences between the values of climatic factors in different periods raises the question of the possible variation of climate by a greater amount in longer time. I therefore propose to make brief statement of the various hypotheses that have been offered to account for possible variations of climate in long periods, commonly known as secular or geological variations.

The question may be approached in two ways. We may inquire of the geologist what he has found that indicates climates during past stages of the earth's history, different from those which it now enjoys or suffers: or we may examine the physical basis of climate, discover its various controls, and then consider which of these may vary, and discuss the changes of climate that will follow such variations. The problem deals necessarily with hypothetical matters; but it suffers not at all from this, except in the minds of those who confound hypothesis and fact. The whole study of climatology and its associate, meteorology, demands fertility and ingenuity of hypothesis, without which it would be far behind its present well advanced and reasonably established position.

Geological evidence of climatic change in the past is not wanting. One who contemplates the excellent quality of the evidence on which this statement is based must conclude that our present climate, which appears to be constant from decade to decade, is in truth a slowly changing variable. The first fact that strikes us is the extraordinary record of a glacial period; a time when large parts of northwestern Europe and northeastern America were invaded by sheets of ice from certain higher regions of relatively abundant snowfall. Products of this period are plainly visible on all sides of this city of Providence; and if your state were stripped of its glacial deposits, it would lose a considerable share of its present modest area. Synchronous with our complex glacial period, there were stages of high water in certain enclosed interior lake basins, of which those of Utah and Nevada have been best studied. At present the beds of these lakes are parched sterile deserts. Some have concluded that a climate thus giving glacial records in one region and pluvial records in another must have been a climate of higher mean annual temperature than now, because the greater precipitation that they postulate is supposed to depend on greater evaporation, and this in turn on higher temperature. Others conclude that glacial period means a time when snowfall began

earlier in the fall and lasted later into the spring; and that a pluvial period was not so much one in which more rain fell, as one in which less rainfall was carried away by evaporation: that is, both the glacial and pluvial record is here taken to mean a lower mean temperature than now prevails. This seems to me the better conclusion; and I believe it is supported by the evidence derived from fossils associated with glacial deposits, which indicate a more boreal fauna than now inhabits their region.

The glacial period of which we know most is one of very recent geological date; so recent that its topographical records still preserve their freshness of form; so recent that even a great river like the Niagara has not, since that period, been able to cut a gorge more than six or seven miles back from the bluff over which its cataract first plunged. But geologists tell us of signs of glacial action in deposits of much more ancient date. Fontaine describes conglomerates in the Triassic formation of Virginia of a coarseness that he regards as beyond the transporting power of water. South Africa is reported to contain old conglomerates that closely imitate glacial deposits. In India and elsewhere great boulders are found in fine-grained marine sediments, indicating transportation by icebergs in a sea where no bergs now float. These facts and others of the same nature call for explanation; but it should be recognized that they do not at present demand the assumption of a broad area glaciated at one time; while it is true that broad glacial records are relatively evanescent, it seems that the known ancient records might in most cases be the product of conditions as local as the present glaciers of Switzerland or of New Zealand; and as such they would indicate local glacial climates dependent on an ancient mountainous topography near their place of occurrence, and not a broader glacial climate, dependent on the combination of various external factors.

Contrasted with the records of colder climates, we read of fossil plants of decidedly temperate or subtropical character in Greenland. These indicate a once warmer condition in those high latitudes, and require us to consider secular increase as well as decrease of the mean annual temperature. Other similar records might be referred to.

It is plain, therefore, that changes of climate have taken place; the amount of change being distinct, though not necessarily so great as one might at first suppose. A moderate cool-

ing in our climate would clothe the White Mountains with glaciers again, and a moderate warming might dissipate the snow fields of Greenland.

Let us now take up the other side of the problem. On what does climate depend; what are its physical controls; and how far may these controls be supposed to be variable. The final consideration of the problem of secular climates can hardly be entered upon. We are not yet able to define exactly what changes have taken place in the controls of climate, and demonstrate how far these changes will account for the facts discovered by the geologist. Hence the strongly hypothetical quality of the study at present.

The climate of a place certainly depends in a definite manner on many factors. Stated algebraically, climate may be defined as a function of many variables. We should try to discover all these variables, to find out how far each one may vary from its present value, to measure the climatic effect of each variation, and finally to work out the resultant effect of the combined variation of all. The variables now recognized are first those connected with the sun, its activity and its outer atmosphere; second, those connected with the earth as a whole, the form of its orbit, the attitude of its axis, the period of its rotation, the quality and quantity of its atmosphere; third, those dependent on the larger or geographic arrangements of its lands and waters, its ocean currents, its mountains, plateaus, basins and low lands, its continents, coasts and islands; fourth, those arising from some change in atmospheric processes, such as storms, etc.; fifth, those dependent on the immediate, detailed, local or topographic surroundings of a station. The variations in the "temperature of space," and in the relations of the moon and the planets to the earth are omitted from this list, because there is no good reason for their inclusion.

The activity of the sun varies locally in its dark spots and its bright eruptions. The spots are known to be more frequent about every eleven years. Many stars are known to vary regularly in brightness through periods of different lengths; others vary irregularly; still others suddenly blaze out in the sky and then fade away. Such accidents may characterize that bright particular star to which we are so much attached. All rational hypotheses that have been framed to account for the sun's heat include conditions that will cause a slow variation in its temperature; a paradoxical increase of temperature, if it be supposed

to be or to have been a mass of self-compressing, cooling gas; a decrease of temperature, if it be regarded as a cooling, contracting globe of mixed constitution. It is on the latter basis that Thomson, Tait and others have sought to set limits to the time available for geological processes, somewhat to the embarrassment of geologists; but the limits pertain more logically to the postulates accepted at the beginning of such calculations than they do to the actual one and only true history of the earth. The sun is surrounded by a vast gaseous atmosphere, ordinarily invisible to our eyes; a variation in the constitution of this atmosphere might, as Langley has suggested, alter the rate at which the heat of the sun is emitted. But while we really know so little of the sun, and so little of its vastly long history, hypotheses as to its past and future are highly gratuitous; their operations are slow and uncertain; they need not hasten the most deliberate geological speculations.

The astronomical relations of the earth and sun are continually changing by reason of the disturbing action of the moon and the other planets. The measure of the rate and the amount of these changes is generally taken from Leverrier's calculations: I am not aware how far these have been independently recalculated by others, how far their calculation requires the simplication of formulae by the omission of the smaller quantities, or how far the data on which the formulae are based may be altered by new and more nearly exact determinations; but when astronomers tell us just how many years have passed since the earth last had its northern winter in aphelion at a time of maximum eccentricity, I have been tempted to ask what is the possible error of such determinations. They are doubtless more definite than the physical measures of the same age; but are they not open to considerable alteration?

Croll has more than any one else called attention to the climatic effects of the possible combinations of variable eccentricity of orbit and variable attitude of the earth's axis, maintaining that if the northern hemisphere was turned away from the sun at the aphelion point at a time when the orbit was as eccentric as possible, the resultant cold of our long, severe winter would, in combination with various associated meteorological processes, overbalance the heat of the brief torrid summer, and a northern glacial period would ensue. This most ingenious theory found much acceptance at first. One uncon-

scious argument in its favor was perhaps the confusion that the geologists of twenty or thirty years ago felt on discovering a cold fact that they could not account for; in their desire for an explanation of it, many of them accepted Croll's theory somewhat too hastily. Now they seem to be more accustomed to living alongside of unexplained facts, and they are less hasty in settling down upon theories to account for them. Apart from certain physical errors that Woeikoff has pointed out, Croll's theory implies an unproved frequency of glacial periods, an undemonstrated alternation of glacial periods in opposite hemispheres; it calls for a more symmetrical distribution of the glacial sheet around the north pole than was the case in the last glacial period, for in both Norway and northern Canada, the ice sheet moved poleward; it depends on the exclusion of the equatorial ocean current from the north Atlantic, and this seems extremely improbable, even if winds alone cause ocean currents, and if differences of polar and equatorial ocean temperature are ineffective in causing a concretional circulation, such as Ferrel and others maintain. On the other hand, James Geikie and others maintain that when the physical conditions of Croll's theory conspire favorably with the varying relations of shore-lines, ocean currents, continental elevations and so on, a glacial period must ensue.

Astronomers tell us that while the axis of the earth turns in various directions, it stands at an essentially constant angle with the ecliptic. Geologists would be delighted to tilt the axis in other ways, so as to vary the intensity of seasonal alternation; or still better, to shift the axis within the earth, so as to bring the poles to new geographic positions, as old Halley imagined had been the case, when he explained the severe climate of New England as perhaps a remnant of the cold it had caught when the pole was hereabouts. If we could only shift the pole to Iceland or southern Greenland, a fine glacial sheet would spread over the regions where we find glacial records; the equator would then lie south of Cape San Roque, and the great volume of the equatorial current would remain in the southern hemisphere; the Sahara would be in the temperate zone and would have rain enough to wash out its wadies. This would be very accommodating; but there is not the least reason to suppose it ever was true. Like Croll's theory, it does too much; if the axis shifted once, it would shift often, and we should expect to find more glacial records than are known.

The rate of the earth's rotation is supposed to have decreased in the course of the geological ages, by reason of the interaction of the earth and moon. Ferrel and George Darwin have demonstrated the validity of the processes involved, and Ball has popularized them in a delightful lecture, poetically called a "Glimpse Through the Corridors of Time." With a faster axial rotation there would be greater delay in the atmospheric convectional interchange between equator and poles, and hence greater contrasts of torrid and frigid temperature; but it was long before the recent glacial period that this process caused any appreciable change from the present state of things; it is a weak and slowly changing process compared to many others; its further operation can make no significant change in the future climatic zones of the earth.

The radiant energy of the sun travels away in straight lines at an inconceivable velocity. An insignificant fraction of it falls on the planets; the rest is "wasted" in space, giving to other stellar systems only a starlight's worth of energy; and, as far as our experience goes, the planets of the other stellar systems could get along very well without this minute supply; their astronomers would have one less star to catalogue if our sun "went out," and if all the stars but their own sun were extinguished, they would, to be sure, have had no trouble from astrologers and they would have less easy means of determining time and latitude; but in other ways they would, as far as we know, get along very well, just as we should if there were no stars in the sky. But when saying that almost all solar energy is thus "wasted," the statement must always be qualified by adding, "as far as we know"—and that is not very far.

The insignificant fraction of solar energy that falls on the earth is our most important climatic factor by far. Any variation that it may suffer, any change in the process by which it is transformed into heat, and any rearrangement in the work that the heat has to do on the earth, must affect our climates. It first encounters our atmosphere: if the gases and vapors of the atmosphere vary, the temperature that will be maintained at the bottom of the atmosphere must vary also. The atmosphere has undoubtedly varied in the course of ages. When the earth was hotter, the amount and variety of vapors in the air must have been greater; but this stage is long past, even geologically. Volcanic action gives forth much water vapor and carbonic acid; the ocean contains a large amount of atmospheric gases, particu-

larly carbonic acid; the plants of various coal and lignite deposits have abstracted much carbon that once, but probably not all at once, existed as carbonic acid in the air. The rocks have rusted, thereby diminishing the supply of free oxygen. But in all this, we find no likelihood of such variations as would cause quick alternations from glacial to milder climates. The variation in the quantity of water vapor is a function of temperature chiefly, and not spontaneously initiated: as vapor, it does not appear to be as active in determining terrestrial temperature as Tyndall has supposed; but as condensed vapor, that is, as cloud, fog, mist, haze, it is of extreme importance: witness the part that Croll calls upon it to play in his physico-astronomical theory of climate.

Lyell was the latest advocate of great geographic rearrangements as a possible cause of climatic change: but the further study of the continents and ocean basins has led most geologists to accept the suggestion made by Dana, that we have no reason to suppose such complete rearrangement of land and water as Lyell assumed. It would be a very potent method of effecting climatic change; and there is no question that among the possible rearrangements that one might devise, there could be invented a distribution of land and water that would be vastly more favorable to human population than the one that many of our fellows have to contend against. If Asia could be subdivided so as to break up the vast monotony of the Pacific; if some of our barren polar lands could be placed in the watery southern temperate zone; if the African coast line could be indented; if the mountains could be placed on the leeward side of the lands; and particularly if a more frequent branching of equatorial currents towards the pole could be introduced, we should be better off: and if these changes were within the reach of human endeavor, they might profitably be undertaken to-day, just as we undertake to cut down forest overgrowths, to drain swamps, to fill bays, to bridge rivers. But such changes as these need not be considered in any actual climatic problem: we must postulate only moderate changes. They are of two kinds: one involves a change of altitude; the other a change of area. The most important application of the latter is the possible alteration in the course of certain ocean currents; for example, the admission of a North Pacific current into the Arctic ocean; Greenland might then deserve its name: or the withdrawal of much of the Gulf stream from the north Atlantic by opening a

deep and wide passage at Panama, or by shifting Cape San Roque. How far such changes have gone is unknown; but it seems likely that the admissable amount of variation would be of the greatest importance in the discussion of ancient climates. The boulders in ancient Indian strata may perhaps have been carried there by currents unlike those we now find.

Besides the effect just considered, there is the alteration of water area by the submergence of broad lowlands, such as the plains from the Caspian sea to the Arctic ocean, or from the Gulf of Mexico to the Arctic; while there is no reason to think that these were abyssmal oceanic basins, there is reason to think they have been overflowed, and that at no distant geological time. Such changes would have most important consequences on the lands of the submerged regions. Professor James Geikie has lately summarized the admissable consequences of this kind in an excellent essay.

Change of altitude is important in controlling precipitation as well as temperatnre. There would have been no glacial record in Switzerland if there had been no Alps; hence Charpentier long ago ascribed Swiss glaciation to a former greater altitude of the mountains there; hence Bell, in his recent discussion of the glaciation of Canada lays much emphasis on the possible quarternary variations in the elevation of the Laurentian Highlands; hence Upham lately advocates broad elevation as the chief promotor of the glacial period; hence it may be inferred that a large part or the whole of the cause of the Triassic glacial conditions inferred by Fontaine in Virginia was to be found in the much greater height of the Appalachians then than now. But while elevation may provoke glaciation, and depression or denudation may discourage it, geologists would not as a rule be satisfied with a purely geographic cause of this kind for the recent glacial period. It is important, but it is not enough alone. It is pretty well shown now that the glacial period consisted of two glacial epochs, separated by a milder epoch; a considerable degree of accordance between records thus complicated is found in regions widely separated; and this could hardly be the case if a geographic cause of the kind now considered had been alone operative. Some additional, external causes must still be sought for. As to denudation, it would certainly in time reduce mountains from a glacier-bearing height to a more temperate elevation; mountains are raised and razed; in a geological sense, they are ephemeral forms; but the

climatic cycle is much shorter than the geographical cycle. I do not doubt that the wearing away of mountains has been effective in deglaciating certain ranges, but the process is too slow to account for the disappearance of our recent ice sheets, in the manner suggested by Tyndall. The Alps have not greatly changed their form from preglacial time to the present day.

The growth and waste of mountains must greatly effect the climate of districts lying to leeward of them. Thus our Great Basin in Nevada and Utah must be drier now than before the rather recent elevation of the Sierra Nevada; but here again, the cycle of geographic change is too slow to account for the much briefer cycles of climatic changes. The old lakes of the Great Basin, Bonneville, Lahontan, and Mono, have been twice filled and emptied by climatic changes since the Sierra had substantially its present altitude; at least, changes in the altitude of the Sierra sufficient to account for the pluvial and dessicated periods indicated by the old lake sediments are not suggested by Gilbert, Russell and others who have described that region. Here again, some external cause is called for. Smaller geographic or topographic surroundings are unquestionably of importance in climatic study, but they do not seem to afford explanation of the broad variations of climate here considered.

Thus far, I have considered chiefly the possible differences of temperature, and have alluded only incidentally to other climatic factors. This is fair enough, for differences of temperature are at the root of all other atmospheric conditions. When these change, the others follow. But inasmuch as some of the most distinct records of ancient climatic change are concerned with variations in the amount and kind of precipitation, some consideration should be given to that important subject.

One of the most important generalizations of modern meteorology is that one which shows that by far the greatest part of the precipitation in temperature and higher latitudes accompanies cyclonic storms. All our winter rains and snows are of this origin; our summer thunder-storms are generally closely associated with and dependent upon cyclonic circulation. The exceptions to this rule are such as the summer afternoon thunder-storms of hot weather in low regions, or the summer diurnal thunder-showers of mountain peaks, from neither of which is much snow derived; the conditions that foster such local, convectional storms are moreover such as favor active evaporation leading to a dry and not to a pluvial period. In the

torrid zone, where the processes dependent on the diurnal variations of temperature are more marked than with us, the regularity of diurnal rains is increased; in the doldrum belt, such are probably the chief causes of rainfall, but even in the torrid zone, many heavy rains are cyclonic; in India, the "bursting of the monsoon" is now shown to be the arrival of a cyclone, and Blanford describes the winter rains as cyclonic storms of moderate energy. Faye, who discredits the similarity of our rain storms with topical cyclones, thereby departs from the admirable generalization to which Redfield gave early and emphatic statement.

In all the countries where extensive glacial records have been discovered, the snowfall is essentially, I may say wholly, cyclonic. It has already been stated that the first consideration for the product of an ice sheet is that the season of snowfall should begin earlier and last longer than it does now; then some of the winter snow would last over the summer, and thus initiate the storing up of a supply that would accumulate to a greater and greater depth, until it finally acquired thickness enough to move away and invade districts where local accumulation of snow was impossible. Can we devise any atmospheric arrangements by which to increase the snowfall from the great circumpolar procession of cyclonic storms that is continually marching over Canada and Norway, the two countries of extensive recent glaciation? Omitting all changes of the geographic kind, can we conceive any processes that would deliver a greater annual snowfall to these countries? It must be remembered that all the vapor which the atmosphere contains in excess of that which suffices to saturate it at the freezing point, is useless in the production of snow, as Dutton has so clearly shown: it is for this reason that an increase of mean annual temperature, as advocated by Lecoq and others, appears to be ineffective in producing permanent snow-fields in countries where no snow now lasts through the summer. The only manner in which a higher mean annual temperature might assist snowfall would be in ensuring that there should be enough vapor present always to saturate the air when the temperature fell to freezing: but this is a deceptive assistance; for the winds that we now experience at temperatures below freezing and still unsaturated are as a rule not those of the wet part of our cyclones, but of the dry anticyclonic areas. It would be difficult to imagine how a rise in the mean annual temperature would moisten these winds; while there can

be no doubt that such a rise would increase the amount of rain and decrease the amount of snow given out by the wet cyclonic winds. It is very difficult to imagine any process that will produce more cyclonic storms, or make those that exist more active, or cause them to linger longer on the once glaciated regions; hence the only way in which Canada and Norway can gather snowfields appears to be not by raising but by lowering the mean annual temperature, so that more cyclones shall then than now cause snowfall, and fewer of them give rain. The desired reduction of temperature can be produced by less activity in the sun, by placing winter in very eccentric aphelion, by a diminution in the quantity of our atmosphere or in its heat-preserving power, by shifting the pole, by diverting the warm currents, or by raising certain lands: and the question still remains, by which one of these several agencies or by what combination of them was the quaternary glaciation produced. Secular variations of climate have undoubtedly taken place, but we cannot give specific explanations of them.

Instead of at present searching too anxiously for such specific explanations, I believe that work can be more profitably applied to the closer determination of the facts that call for explanation, and to the better definition of the processes that may have a share in the explanation. The astronomer may advantageously continue to study solar physics; the mathematician may review the calculations concerning precession and eccentricity; the physicist may investigate the action of our atmosphere on solar and terrestrial radiations; the geographer may review the causes of the ocean currents; the glacialist may inquire further into the nature of the glacial record; the palœontologist may still collect and discuss the evidence of his fossils; the geologist may try to define more accurately the variations of land and sea in past times. When fuller information is gained on all these points, better answers can be given to questions about the secular variations of terrestrial climates.

Prof. Wm. H. Niles, president of the Society, said that geographic changes, such as the increase in continental and oceanic areas, were very important and even small changes had a great effect. For this reason, climate is not disposed in zones as the normal climate would be. The climate of the glacial period was probably less cold and more humid than is generally supposed. A change in the distribution of the species of plants should not be regarded as certain evidence of a change of

climate, since the species themselves are rendered more or less hardy by these surroundings. The life of plants is often less affected by the extremes of climate than by the means. While admitting the necessity of astronomical changes to produce the great climatic variations, yet the author believed much has been effected by geographical and geological forces acting unequally.

Prof. E. Douglas Archibald, of England, who was a guest of the Society, after relating the influence which Lieut. Maury had in turning his attention to meteorology, declared himself a disciple of Croll in respect to the theories which had been discussed. In India, Prof. Hill and himself had, years ago, noted a marked variation of the winter rainfall and temperature during a sun-spot period. The prevailing opinion is that more heat is received during a sun-spot maximum, but the difference is small, and the speaker held the contrary opinion as a working hypothesis. Extensive actinometric observations in different parts of the world would settle this quantitatively. The ice spoken of by Prof. Davis as flowing northward from Labrador might have been caused by the fact that there was no high land to the southward. The speaker considered ocean currents to be a powerful factor in a climatic change, and any deflection in these currents caused by slight geological changes might have been followed by results out of all proportion to the primary cause. Floating ice is another climatic factor, as is shown by the marked cooling during May and June, which occurred over Europe in years when the Atlantic was most full of floating ice. The speaker agreed with Prof. Davis that the questions involved should be considered separately, as the mind is unable to grasp so many variables at once.

2. *Supposed Recent Changes in Climate.*—Prof. W. Upton introduced the subject by alluding to the forces at work to produce climate, and to the fact that the slow changes going on would be apparent in climatic changes only after a long series of years. The sun's heat, however, the prime source of climate, is probably subject to fluctuation, resulting from the well-known fluctuation in the solar activity, but it was not yet established whether any of the observed fluctuations in climate could be directly traced to that cause. As the theoretical study of climate failed to point out the changes which might be looked for, it is well to examine the records themselves to see what their testimony is. The records at Providence and New Bedford had been specially studied and showed great fluctuations in different

years, with some indications of periodicity but no progressive change. How then can the universal popular belief, that the climate is changing, be accounted for? It is for instance widely believed in southern New England that the winters are milder and that there is less snowfall, than formerly; that the winters begin later and the springs later also, so that December is warmer and April cooler than formerly. These and similar beliefs can be explained by the short and defective memories of people, who recall a few seasons only, and who exaggerate the frequency of some special event; also by a change of residence, the person forgetting that there are great differences in localities separated by a short distance; also by the fact that the fluctuations are large and often in the same direction for several successive years; also by the difference between the impressions of the child and the adult.

The records at Middletown, Conn., for thirty-one years, sent by the observer, H. D. A. Ward, Esq., were then examined. The fluctuations in annual temperature, annual precipitation, and annual snowfall were exhibited on a chart, and shown to be large, with a slight indication of periodicity of about twenty years, but no progressive change.

The record of the closing of the Hudson River to navigation at Albany for a century, 1790-1890, prepared by Mr. E. B. Weston, was also exhibited, showing that the present dates are certainly no later than formerly.

The subject was continued by Prof. N. F. Davis, of Brown University, who exhibited charts prepared from the records at Providence since 1831, and at New Bedford since 1813, the former kept by Prof. Caswell until 1876, and by the City Engineer since that date, and the latter by Mr. S. Rodman and his son, Mr. T. R. Rodman. After alluding to the inherent uncertainty of such records arising from changes in location of instruments, in the instruments themselves, in methods of measuring snow, etc., Prof. Davis explained the charts which illustrated the fluctuations in annual temperature, annual precipitation, temperature of the three summer months and of the months of December and April. The chart of annual temperature showed fluctuations of about 6° or 7° F., with no progressive change, but with a marked indication of a period of about twenty years. The chart of annual precipitation showed great fluctuations, the maximum amount being double the minimum; the Providence record showed a marked progressive increase,

but it was evident from the longer record at New Bedford that the Providence record began at a minimum, and as the precipitation was now at a maximum, the progression was probably only apparent. There was the same indication of a twenty year period in the precipitation record as in that of temperature. The chart of the temperature of the summer months showed also a periodic fluctuation, the last few years having been near a minimum. The temperatures of December and April showed no progression, and failed to confirm the popular belief that the former month was growing warmer and latter colder.

Mr. H. Helm Clayton, of the Blue Hill Observatory, exhibited curves derived from the observations at Gardiner, Maine, from 1836 to 1885, Milton, Mass., (1849–'89) and Cambridge, Mass., (1840–'80). The two latter show a continuous rise in the temperature of about 1.5° F. for each decade, but the first named station shows an equally marked fall in temperature. This might result from the observations commencing with a maximum and ending with a minimum period, but the probable error is less than the actual change. The curve of precipitation at Gardiner follows closely that of temperature. The Milton temperature record is remarkable from having been made with the same thermometer exposed in the same place and observed at the same time by one person during the forty years.

Lieut. J. P. Finley, of the U. S. Signal Service, spoke on tornadoes with special reference to the recent one at Louisville, Ky. He described the typical distribution of pressure and temperature giving rise to tornadoes which occur in the southeastern quadrant of an elongated area of low pressure, having its major axis southwest and northeast, and with the apexes of the isothermal lines representing areas of warm and cold air in the southern and northern quadrants, respectively. New England, the speaker said, was within the tornado belt, but not within the battle ground. The cyclones causing these storms generally came from the North Pacific region, the path of the tornado only being local. The cyclone which induced the Louisville tornado came in on March 24 from the North Pacific and traveled several days, feeding on heat and moisture. The Johnstown, Pa., deluge of last year was to be explained by the indraught of warm, moist air from the Gulf, while the cyclone remained stationary.

TROMBES AND TORNADOES.

By H. Faye,
Membre de l'Institut, Président du Bureau des Longitudes, etc.

(CONTINUED FROM MAY NUMBER.)

Direct Study of Tornadoes.—I take for the basis of this study, the reports of Mr. Finley. These reports are made in a truly scientific spirit and without prejudice. In reading them one is convinced that the author had the right to say: "I have done my best to collect facts only, banishing from my mind all theoretical predispositions."

In spite of this impartiality which is everywhere visible and which gives so much value to Mr. Finley's report, it is well to know what is his real opinion. His opinion is that of all meteorologists who have adopted Franklin's theory, perfected by Mr. Ferrel. This is easily seen by one phrase in the little book on tornadoes, alread cited:

"Owing to the great centrifugal force of the tornado-cloud, the center of the vortex must very nearly approach the condition of a vacuum."

According to this Mr. Finley ought to be disposed to search for the effects of which we have spoken above, and in particular to examine whether the houses destroyed were destroyed by explosion, that is, by the sudden expansion of the air contained in them. This examination is, besides, imposed by the instructions of the Signal Office;—witness article 27 of the tornado circular No. 1, New Series: "In the destruction of your buildings did you notice anything in the disposition of the debris after the tornado-clouds passed that would indicate the effect of an explosion, as for example, the sides and the ends of a building being thrown outward and the roof carried off or let down upon the floor?'"

Mr. Finley does not fail to touch upon this point in his report: "I have particularly directed my investigations toward manifestations of this character, but without being able to find the least trace of them. Houses placed in the track of the center of the hurricane, have been torn in pieces almost instantaneously; others situated a little to the right or left, in which might be encountered such effects, had generally doors and windows open, on account of the heat of the day; they could only be closed in part before the storm began its work of de-

struction. Had the houses been closed as would have been the case in cold weather, there would undoubtedly have been some effect due to the rarefication of the surrounding air. The mass of enclosed air, being no longer maintained by external pressure, would have re-established the equilibrium in projecting the roof and walls suddenly in different directions."

Preconceived ideas have had no effect upon the report of Mr. Finley. This may be seen on the occasion of the passage of the tornado of Delphos on Blue River. It is well to examine this opinion at once, because it has influenced singularly the stories of certain witnesses whom Mr. Finley was obliged to consult.

First, the authors of the theory of the almost complete vacuum, which tends to form in tornadoes,—a vacuum which should act as in the experiment of the cupping glass of physicians,—have not thought, evidently, what effect this vacuum would have upon animals and man. When a small animal is placed under the bell of an air-pump, it dies at the end of a few strokes of the piston. An almost instantaneous vacuum, capable of wrecking a house, could not fail to produce powerful effects upon man. The case of the French aeronaut, Croce Spinelli, who succumbed to a rarefaction less complete, and less rapid (at about 8,000 meters of altitude), shows what would happen to persons subjected suddenly to the almost complete vacuum of a tornado. Now the persons over whom a tornado has passed and who have been killed have met their death by the violence of the wind which threw them to a distance against some obstacle, or by the force of debris projected with an unheard of force in an almost horizontal direction, and never by the vacuum. The survivors have related what they felt at the first shock of wind, a sensation of intense heat, as if suddenly placed before a furnace, and, remarkably enough, this sensation of painful heat was succeeded by an entirely opposite one, that of intolerable cold. This succession of effects has nothing to do with the action of a suddenly produced vacuum. It is easily explained, the heat, by the shock of a mass of air animated by the enormous velocity of from 100 to 200 meters per second; the cold, by the relatively very low temperature of the descending mass of air of the tornado.

No one mentions the sudden suffocation of a person deprived of air, nor of the painful sensation which should follow the

reduction of a weight of the atmosphere sufficient to wreck houses. *

To complete what has preceded I can do no better than to refer the reader to a note published by Mr. H. A. Hazen in No. 3, July, 1889, of this journal. Notice especially Nos. 8 and 9, pages 102 and 103.

On the Destruction of Property by Tornadoes.—According to the preceding theories tornadoes act by means of violent centripetal and ascending currents: according to mine, by simple descending gyrations, with a vertical axis.

These two modes of action are to be compared with the facts demonstrated by the investigations and charts of the Signal Service.

In the first theory the centripetal impulsion of the air may destroy buildings and tear up trees, leaving on the surface the heavy debris. But there is a second action. Centripetal currents rise on passing through the foot of the *trombe.* On rising they carry away light debris, and it is said, even beams, roofs, bars of iron and masses of water, whirling them upward in the tube as far as the mouth. The tornado carries them in its mass for some distance in the direction of its motion.

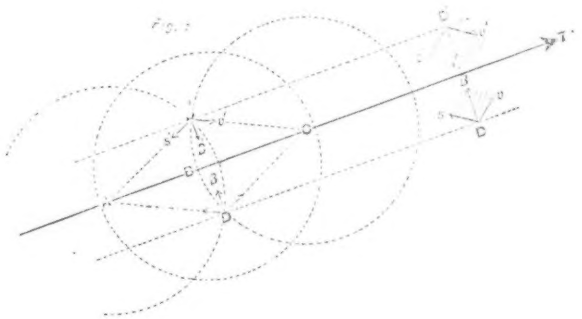
This last characteristic is completely wanting in the tornadoes such as are presented in the second theory; these do not transport to a distance in their tubes or in their mouths, a leaf of tree, nor a drop of water. In this theory the descending gyrations attacks obstacles almost horizontally; destroys houses and leaves on the surface all heavy debris. The less massive debris beams, vehicles, water-pipes, etc., are projected with violence almost in the direction of the gyrations, but doubtless not so far as cannon balls, or even a rifle shot. In the same way such debris, as leaves, straw, sand, etc., are thrown around the *trombe,* but the resistance of the air soon forces them to fall again; they form a sort of a cloud of dust mixed with condensed water and vapor in the form of fog.

Figure 1 represents according to the first theory, the succession of currents, or the impulsions which fixed obstacles will

* As to the witnesses who, before the arrival of the tornado, felt a difficulty in respiration: ("I felt a want of breath—;" "The air frequently appeared too rarified to breathe freely." . . . . "It was terribly oppressive; it seemed as if the atmosphere was unusually heavy and pressing down on me with a great weight," etc.) . . . These sensations precede the arrival of the tornado by an interval of some length and are easily explained.

have experienced at D and D', on the passage of a centripetal tornado progressing over the trajectory $ABCT$.

The obstacle D will receive the winds directed toward the moving center, and designated by arrows Dks, DB, Dk. All the impulses will be comprised in the section sDu. If there is debris, it will be projected into the interior of this sector. For another obstacle D' situated the other side of the trajectory; the sector of impulsions and of debris will be, like the first, directed toward this line, but in a symmetrical position.



To account pretty nearly for the progressive movement of the tornado, it will suffice to incline the two sectors sDu, a little in this direction $s'D'u'$. Finally if any debris projected in the direction D, receives other impulsions, these will not tend to make it leave its sector.

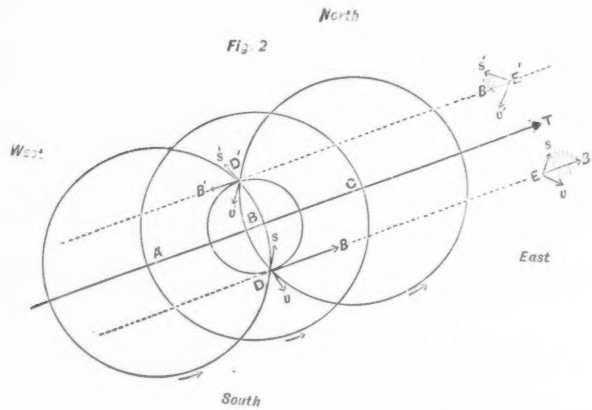
Figure 2, relative to the purely gyratory theory is quite different. The winds felt in D, such as DS, DB, DU, are found in a sector of which the opening is in the direction of the trajectory, at 90° from its homologue in the preceding figure. In D the sector is directed in a direction contrary to the advance of the tornado. But here the velocity of the translation must also be taken into account. There is a dangerous border, as in cyclones, the place where the velocity is added to the impulsion DB, and a manageable border, where the velocity is subtracted from the impulsion DB.

Farther, such objects as D situated on the dangerous side (south of the central trajectory $ABCT$), and thrown ahead in the direction DE with a velocity greater than that of the tornado, will soon be stopped by the resistance of the air and will

fall to the ground at *E*. There it will be taken up again by the tornado and will receive new impulsions in the same direction. Under favorable circumstances and configuration of the surface, these impulsions could be repeated several times in such a way that the debris *D* would pass over distances such as could not be explained by a single impulsion. But this phenomenon will not be produced except in the direction of the trajectory, that is north-east. It is only in this direction that the objects carried from a house may be looked for.

It might happen too that the objects projected at first in the direction comprised between *DS* and *DB* would be taken up again by the tornado and undergo new impulsions directed to-

Fig. 2



wards the trajectory in such a way as to pass to the other side (to the north). There the winds having directions opposite to those of the tornado will disperse the debris in the arc of a circle or in a spiral.

It must not be forgotten that the gyrations of a tornado are in helecoidal spirals, more or less oblique to the horizon. Objects which are slightly elevated will be projected with this inclination. But the gyration is destroyed by contact with the ground, the air escapes in all directions around the tornado in the series of its tangential planes, rising at the same time by a sort of reflection upon the ground, and the objects thus projected may have a greater range.

Here are both theories; let us study now the facts collected

by Mr. Finley in his investigation of the thirteen tornadoes of May, 1879.

Lee Summit Tornado.—Let us first examine the plan of destruction of the home of Mr. Hutchin's, situated at the right (or to the south) of the central trajectory. The house was demolished almost in an instant, and its debris covered a sector of which the opening is turned (save one arrow a little turned aside) in the direction of the trajectory. Heavy pieces of tin pipe, iron spouts, etc., were found at great distances to the NE of the house. Thrown with extreme violence they act like projectiles. Out of sixty trees in the garden, fifty-seven were uprooted and thrown down towards the NE.

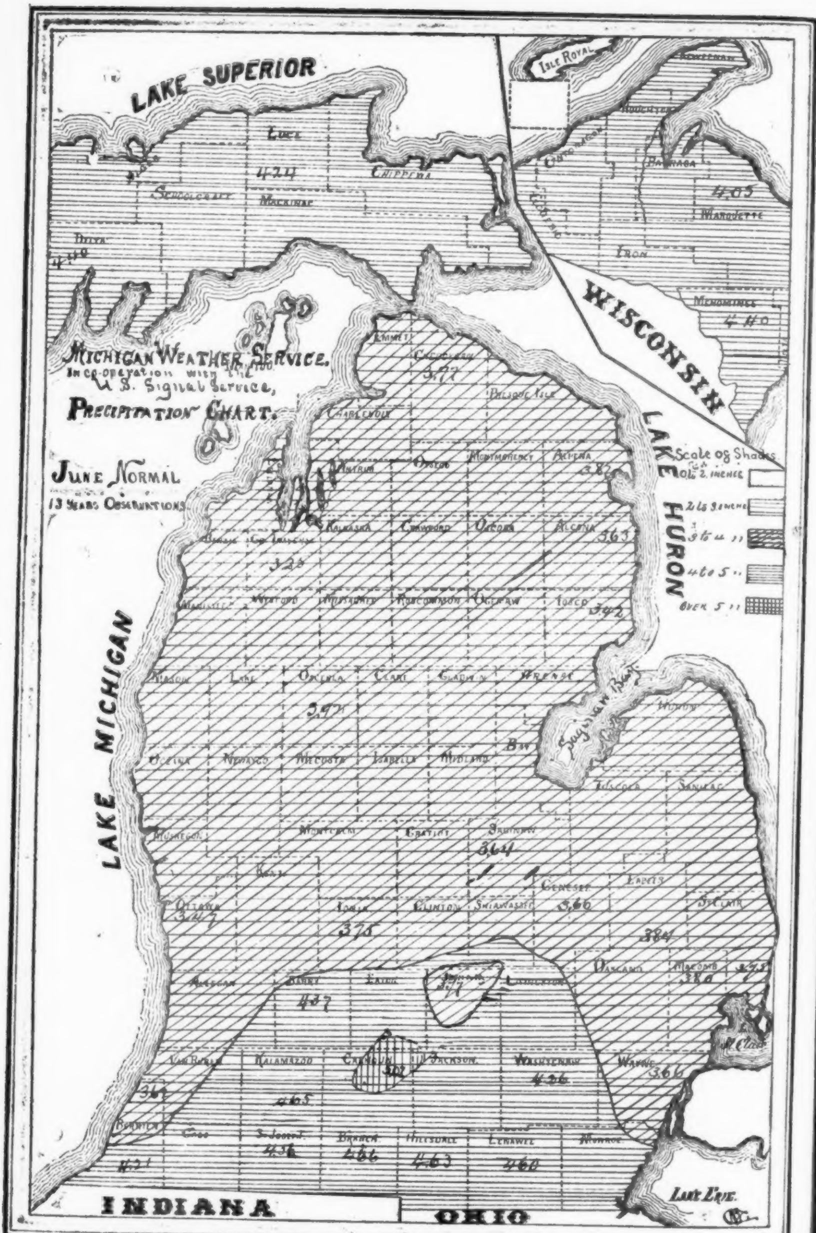
'After having destroyed Mr. Hutchin's house, the tornado entered a bushy grove at the bottom of a ravine. It had only to descend a little to continue its ravages. The arrows in Mr. Finley's chart indicate trees leveled. At the right of the central trajectory (dangerous side) all the arrows lie in the direction of the trajectory or at least in the sector *SDU* (Fig 2). At the left, the safe side, the trees fell in the sectors *S'D'U'*, opposite to the path of the tornado. Upon the trajectory itself, they fell crosswise, some one way and some another.

[TO BE CONTINUED.]

RAINFALL IN MICHIGAN—JUNE.

BY N. B. CONGER,
Director State Weather Service.

The month of June ushers in the rainy month of the year in Michigan, the average for the state being 3.88 inches, and of this average, the largest portion of it is deposited in the southern three tiers of counties and in the upper peninsula. The average of the first three southern tier of counties is 4.26 inches, and the fall in different sections of the south half of the state has been very heavy, the maximum amount being 10.41 inches in Calhoun county and the least in this section was 1.51 inches. In the portion of the state north of this and south of the Straits, the average amount for the last fifteen years has been 3.80 inches, and is well distributed as to amount and districts. Excessive rainfalls are naturally more prevalent in this month than any other of the year, and the southern section is the portion of the


LAKE SUPERIOR
ISLE ROYAL
MICHIGAN WEATHER SERVICE.
In co-operation with the U.S. Signal Service,
PRECIPITATION CHART.
JUNE NORMAL
13 YEARS OBSERVATIONS.
WISCONSIN
LAKE HURON
LAKE MICHIGAN
Scale of Shades
INDIANA
OHIO
LAKE ERIE.
Luce
4.24
Chippewa
Mackinac
Marquette
4.05
Menominee
4.10
Emmet
3.77
Charlevoix
Antrim
Kalkaska
Crawford
Alpena
3.82
Alcona
3.63
Roscommon
Ogemaw
Iosco
3.42
Clare
3.97
Isabella
Midland
Bay
Tuscola
Sanilac
Huron
Montcalm
Gratiot
Saginaw
3.64
Genesee
3.66
Ottawa
3.47
Ionia
3.75
Clinton
Shiawassee
St. Clair
3.84
Oakland
Macomb
Allegan
Barry
4.37
Eaton
Livingston
Van Buren
Kalamazoo
Calhoun
Jackson
Washtenaw
4.36
Wayne
3.66
4.65
Berrien
Cass
St. Joseph
Branch
Hillsdale
4.63
Lenawee
4.60
Monroe

state most frequently visited by the heavy rainfall. The upper peninsula during this season of the year is visited by frequent and copious rains, which continue during the summer and fall, the maximum being reached in the month of September. The heavy rains of the spring and summer make up for the deficiency of the fall and winter seasons in this section of the state. A peculiarity of the rainfall is that it seems to work up along the west side of the state from the southern section to the upper peninsula, where the larger rainfall predominates during the coming season.

It is a noticeable feature of the rainfall for this month, that at but one station, Port Huron, has the amount for the month been less than one inch, and this amount, 0.55 inch, occurred in 1887.

The lesson of the records would teach that although the amount of rainfall for the previous months may have been below the average, yet the amount for the month of June, is generally sufficient to place the ground in fair condition.

METHOD OF DETERMINING THE DIRECTION OF THE WIND BY OBSERVATION OF THE UNDULATIONS AT THE MARGINS OF THE DISKS OF THE HEAVENLY BODIES.

By Don Vincente Ventosa.*

For the last twenty years, and especially since dynamic meteorology has made such remarkable progress, the attention of meteorologists has been directed to the observation of the upper currents of the atmosphere, the study of which should make known to us the laws which control the circulation of our gaseous envelope and should aid us in the solution, so much desired, of the great, but complex and mysterious, problem of the prediction of the weather.

In fact the anemometers with which the observatories are provided, (without wishing to deny their importance in climatology), are not suitable for the study of the upper currents because they are too near the ground and are subject to the perturbing action of influences which must modify the direction and velocity of the wind, making it sensibly different from that which prevails in the

*Translated from Senor Ventosa's manuscript by the editors.

upper atmosphere. But, on the other hand, observation of the upper currents labors under almost insurmountable difficulties from lack of instruments which can inform us of what happens in these inaccessible regions. To get useful indications we must at least reach as great a height as that of the Eiffel tower, where the diurnal progress of the wind is already quite different from that which prevails at a height of a few metres from the ground, as has been shown in the interesting communication recently made by M. A. Angot to the Academy of Sciences at Paris.

Of all the means proposed, and they are many, for the observation of the upper winds, the only one up to the present time which is capable of regular use and of giving comparable results consists in the determination of the motions of the clouds, these being classified according to their forms and elevations. Quite recently the eminent secretary of the British meteorological office, Mr. R. H. Scott, expressed* the imperious necessity of such observations, while at the same time he regretted the lack of good observers.

Another method, more general in character and particularly useful when clouds are absent, could be based on the study of the undulatory motion which the wind produces on the limbs or borders of the disks of the heavenly bodies which have an appreciable diameter, notably in the case of the sun and moon because of their great angular dimensions. This study, which I believe to be new, reveals some curious facts which I shall now proceed to briefly examine.

When we attentively observe, by means of a telescope, the limb of the sun, for example, we remark that the aspect of the undulations varies from one region on the margin to another. The phenomenon, in its greatest simplicity, appears as follows. There are always two points on the limb, diametrically opposite, where the undulations travel tangentially to it and in the same direction. Passing to intermediate regions, the waves appear more or less inclined to the limb, and at the extremities of a diameter perpendicular to the first, they cut the limb normally.

The motion, which we have just sketched in very few words, indicates by its direction that of the wind which produces it and may serve to determine it, as we shall soon see. In reality what is seen is the relative motion of the wind and the star, for the latter is not fixed, but is submitted to the apparent diurnal rotation of the celestial sphere. However, as the effect of this

* *Nature*, No. 1056, January 23, 1890.

rotation is much less sensible, it can be neglected in most cases, or it can be introduced in the calculation as a simple correction.

In general the phenomenon is more complex. The telescope integrates, so to speak, all the atmospheric motions occurring in the layers traversed by the ray of light and these motions are more or less visible in the ratio of their relative energy. Thus it is frequently seen that two, or even more, independent sets of waves cut and interfere with each other, producing an appearance like boiling in certain parts of the limb. How can these diverse motions be isolated and analyzed? In what way can the respective altitudes be determined?

Since an object appears smaller the farther off it is, it is natural to think that the aerial waves, (if their actual dimensions do not vary considerably with the elevation), when produced by the upper currents, will appear smaller than those due to currents in lower layers; hence they will differ in appearance. It will only be necessary, then, to change the magnifying power of the telescope to make very evident these differences in the appearance of the phenomenon.

Observation confirms this. A highly magnified image often shows the undulations with very short waves much better than those with long waves, while with a feeble magnifying power the opposite is true. The telescope can serve, then, to effect the analysis, to separate the diverse motions of the air, much as the spectroscope separates and analyses the waves which in infinite number furrow, in all directions, the ethereal ocean.

When the sun is observed, it is most convenient and advantageous to receive the image on a screen, and the distance from the eyepiece can then be increased and decreased at will. My observations have been made with a Merz equatorial, with an aperture of 27 centimetres, reduced by a diaphragm to 20. Two images are then seen simultaneously on the screen, one from the large telescope, the other from the finder, and these images have an average diameter of 64 and 20 centimetres respectively. By moving the screen and without changing the eyepiece, the dimensions can be changed from 10 to 90 centimetres,—a scale sufficiently large, in my opinion, to enable us to decide all the circumstances of the undulatory aerial motion.

The large telescope and the finder are each provided with an eyepiece micrometer. The observation consists simply in measuring the position angle of each system of waves by means of a micrometer thread placed parallel to the undulations where they

are moving apparently tangent to the limb. As to the apparent velocity, it is not generally difficult to estimate it by counting the number of seconds taken by a wave to pass from one thread to the next, when the threads are set perpendicular to its direction of motion. It is, of course, desirable to multiply the measurements to eliminate, as far as possible, errors of setting.

I effect the reduction of the observations by the aid of the formulas given by Messrs. Ekholm and Hagström for determining the motions of clouds* and reproduced by Mr. Cleveland Abbe,† introducing into them the parallactic angle at the star. Notwithstanding this slight complication of the calculation, I think it better to use an equatorial telescope, because with it the position angles of the undulations vary with the time more slowly than with the altazimuth, and also because it is easier to apply the correction for the proper motion of the star.

I have also made some observations with the moon. The results were the same, but the feebleness of the light prevented me from receiving the image on a screen. I have also observed the clouds, so far as I have been able, and on the same plan, in order to compare their motions with those of the aerial waves.

I give in what follows the first and more prominent results obtained by this method, the idea of which occurred to me on August 21, 1889, while I was observing, according to my custom, the solar spots:

1. On looking at a star, it is very rare not to find undulations capable of measurement, and the absence of the phenomenon is probably due to a particular state of calm in the atmosphere. On January 14 last, the limb of the sun was exceptionally tranquil, and on that day Madrid was very nearly in the center of an anti-cyclone.

2. When only a single undulation is seen, all the clouds, upper and lower, follow generally the same direction, and this is closely parallel to that of the current measured on the limb. In such cases several superposed waves may be seen, travelling with very different velocities.

3. If there are two currents differently directed, that which corresponds to the higher clouds is betrayed by a very fine undulation, parallel to the cloud-current and moving apparently with some slowness; while that which corresponds to the lower clouds is relatively coarse and rapid.

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* *Mesures des hauteurs et des mouvements des nuages*, Upsala, 1885, page 22.

† *Treatise on Meteorological Apparatus and Methods*, Washington, 1888, page 335.

4. The presence of several undulations at the same time occurs generally when the weather appears unsettled, or when the place of observation is under the combined influence of diverse co-existing centers of high and low pressure. In some rare cases I have thought that I saw signs of a progressive change of direction with the altitude of the current. On the 1st of last November, for example, the wind-vane indicated 180° (the angle counted from North through East); the longest undulation, 276°; the lower cirro-cumulus, 307°; the shortest undulation, 320°; and the superior cirrus, 323°. At that time there was a very deep barometric depression with the center to the North, near Scotland. Similar cases occurred, among others, on the 25th of last November, and on the 3rd and 12th of February of this year. The variation found has always been in the same direction.

5. The lowest wind, that which acts on the wind-vane, will rarely be visible, because its undulation, formed at a small distance from the telescope, will be out of focus. It can, nevertheless, be sometimes rendered visible by focussing for close objects.

6. Under certain circumstances can be seen on the sun's limb a sort of transverse vibration which does not advance with an undulatory movement. This phenomenon is perhaps due to the existence of ascending currents produced by the warming of the lower aerial layers by contact with the soil or in the telescope. Its observation is difficult.

The following table will furnish a sufficient proof of the relation which has been indicated between the movements of the clouds and those of the undulations. The clouds are arranged by altitudes into three classes, and the undulations by wavelengths. The numbers express the percentages of coincidences observed between the two orders of phenomena, taking as a coincidence an angle between the two directions of less than a quarter of the quadrant, or 22°:

	Undulations.			
	Short wave.	Mean wave.	Long wave.	Wind vane.
Superior clouds..........	85%	62%	11%	11%
Doubtful elevation.....	100	100	50	33
Inferior clouds..........	25	82	90	29
Wind vane.............	12	28	30	100

The question can also be presented from another point of view by taking the mean of the angles made by the undulations with the direction of the clouds in each of the above classes: this angle I call their mean *deviation*. The results follow:

	Undulations.			Wind vane.
	Short waves.	Mean waves.	Long waves.	
Superior clouds.........	12°	22°	80°	93°
Of doubtful height.....	10	8	40	75
Inferior clouds.........	62	11	9	51
Wind vane............	102	57	46	0

Although these results are derived from a not large number of observations, they are very significant and, in my opinion, they leave no doubt as to the relative elevation of the currents which give rise to the different kinds of undulations, as well as to the efficacy of the new method for the study of the atmospheric circulation.

This correlation established by what has been said, it is not surprising that there should be another between the undulations and the orientation of the isobars in the place of observation. This is also confirmed by observation, but I have not so far had the time necessary to make the comparison with all the exactness and minuteness which are desirable.

I have also tried calculating the resultant directions observed for each kind of wave. The values found are:

For the short wave N. 48° W.: mean wave N. 52° E.; long wave N. 70° E.

These numbers should be considered as provisional and approximate, being derived from an interval of some months only. However, taking into consideration the rough surface and special position of the Iberian peninsula, the deduced directions are closely in accord with the ideas of several savants, notably with those of Mr. Ferrel who has established the existence of two general currents, of which the upper is everywhere from the west, while the lower is from the NE. in the northern hemisphere.

Finally I must mention a curious coincidence. According to the very interesting observations made on the Eiffel tower and communicated by M. Angot to the Paris Academy of Sciences at its session on December 9, 1889, changes of the weather

appear at an elevation of 300 metres several hours, and even several days, earlier than near the ground. The month of November last furnished a striking example of it.

"From the 10th to 24th of November,—he says,—we had a period of high pressures with calm, or light winds coming generally from the East, and low temperatures. It was only during the 24th that the wind freshened and turned to the SSW., the temperature rose, the sky clouded, and bad weather came on. Now, on the tower, the temperature was still low (minimum, 2°C.) on the 21st with light winds from SE., when, *at 9 p. m. the wind freshened suddenly and turned to the S., then stood steadily SSW.;* at the same time, the temperature, which had been at 2.9°C, at 6 P. M., rose to 6.1° at midnight, and to 9.3° at 6 A. M. of the 22nd."

As it happens, my observations from November 13-21, (except the 19th and 20th which were cloudy), indicate the presence of a single undulation, the azimuth of which took the successive values of 122°, 132°, 120°, 123°, 128°, 134° and 131° the 21st, that is an almost steady wind from the SE. But on the 21st there appeared also another undulation, fine and well defined, coming from the S. (189°), and only visible in the large telescope. As this was observed 10h. 20m., A. M. Madrid time, the modification preceded by more than ten hours, at least, the change of the anemometer on the Eiffel tower.

It should also be noted that on the same day, the 21st, the Paris International Bulletin announced that an important barometric depression was forming to the west of the continent, and that in the corresponding chart, the isobar which passed through Madrid had a north and south direction. On the other side there was still a center of very high pressure, towards the NE. near Vienna, so that it is easy to explain the coexistence of the two observed undulations, the one coming from the S. with very fine waves, the second more coarse and from the SE.

I do not know that these facts are of interest to meteorologists. It may be found that this new method is worthy of trial, and that, in the hands of more skilful observers, it can render some service to the meteorology of the upper regions of the atmosphere, in that it endows the science with a general and easily available means, especially if combined with cloud measures, of determining the problem which Messrs. Ekholm and Hagström call the capital problem of modern meteorology.

MADRID, February 24th, 1890.

CURRENT NOTES.

PROFESSOR FERREL'S friends and correspondents will be interested in knowing that his permanent address will hereafter be Martinsburg, W. Va.

A NEW METEOROLOGICAL SOCIETY.—It is proposed to establish a local meteorological society in New York city, where it is believed that, in addition to the few professional meteorologists in and near the metropolis, there are many other men who are giving more or less attention to weather science, because of the relations existing between some branch thereof and their own vocation. The establishment of a library and the reading of essays by members and outside reports and their discussion would be the chief objects of the society, which would resemble in aim, though on a smaller scale, the New England, Royal, London and Scottish Meteorological Societies. Purely local at first, it might in time become the nucleus of a state or national association of dignity, repute and use. In order to assist in ascertaining how much available material exists for such a society, interested persons are requested to communicate with James P. Hall, editor of the *Tribune*, New York.

THE COLD PERIOD AT THE BEGINNING OF MARCH, 1890.—Mr. C. Harding read a paper on this subject at the April meeting of the Royal Meteorological Society. He said that at the commencement of the month a rather heavy fall of snow was experienced in many parts of England, and very cold weather set in over the midland, eastern, and southern districts, the temperature on the 3rd and 4th falling to a lower point than at any time in the previous winter. The lowest authentic thermometer readings, in approved screens, were 5° at Beddington, 6° at Kenley in Surrey, and Hillington in Norfolk, 7° at Chelmsford and Beckenham, 8° at Addiscombe, 9° at Reigate and Brockham, and 10° in many parts of Kent and Surrey. At Greenwich Observatory the thermometer registered 13°, which has only once been equalled in March during the last 100 years, the same reading having occurred on March 14th, 1845. During the last half century the temperature in March has only previously fallen below 20° in three years, whilst during the whole winter so low a temperature has only occurred in eight years.

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